or the atmosphere, crumbled down." The materials thus supplied went to form the superficial deposits, it being supposed that almost the whole of Scotland was under the sea at the time these changes took place. We feel sure that Mr. Milne Home will get few geologists to agree with him in these conclusions. In the first place, it may very well be doubted whether the faults which cut the strata ever actually showed at the surface in the manner supposed. It is much more probable that the dislocations took place so gradually that any inequalities arising therefrom were planed away by denudation as fast as they appeared. But even were this not the case, it is quite certain that the faults referred to by Mr. Milne Home must date back to a vastly more remote antiquity than the later Tertiary periods. The Scottish Coal-fields, indeed, would appear to be traversed by some faults which, according to the Geological Survey's map and description of the South Ayrshire Coal-fields, do not influence the overlying Permian. It is also indisputable that the igneous dykes, which Professor Geikie has shown to be of Miocene age, are all posterior in date to the faults which shift the Coal-measures. Mr. Milne Home does not take into consideration the prodigious amount of denudation that the palæozoic strata of the valley of the Forth must have undergone in the long ages that intervened between the close of the Carboniferous period and the advent of the glacial epoch. There cannot be any reasonable doubt that the valley of the Estuary of the Forth existed as a valley long before the dawn of the age of ice. But Mr. Milne Home's memoir is taken up chiefly with the history of the drift deposits, which he describes in considerable detail. Especially valuable are the numerous sections given, and the long lists of localities where glacial-striæ, erratic blocks, kaims, and the other phenomena of the drift, may be studied. The author inclines to the iceberg theory of the formation of the boulder-clay, and thinks it may have originated at a time when "the ocean over and around Scotland was full of icebergs and shore-ice, which spread fragments of rocks over the sea-bottom, and often stranded on the sea-bottom, ploughing through beds of mud, sand, and gravel, and blocks of stone, and mixing them together in such a way as to form the boulder-clay." Mr. Milne Home points to the presence of beds of sand included in the boulder-clay as one of several objections to the land-ice origin of that peculiar deposit. He thinks that if the iceberg theory be adopted, the explanation would be simply this, "that icebergs came at different periods, new sea-bottoms being formed in the intervals." But, on the other hand, if the glacier theory be accepted, then it would have to be admitted that the land must have sunk under the sea for every bed of sand we find in the boulder-clay. The author, however, does not seem to be aware that fresh-water beds are found interstratified with the boulder-clay, so that the difficulty in either case is equal. We have not space to notice several other interesting points treated of in this memoir, which contains so many important data, that we can recommend it confidently to our geological readers. We may dispute some of the author's conclusions, but it matters not what interpretation may eventually be put upon the facts, many of the facts are here, and Mr. Milne Home has done good service in bringing them together. J. G.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Phenomena of Contact

IN NATURE for August 24, Mr. Stone controverts two propositions incidentally put forward in a review of Mr. Proctor's book, "The Sun." They are:—

"1. That the irregular phenomena of internal contact of a planet with the sun, variously described as 'distortions,' 'black drops,' 'ligaments,' &c., are not always present, but are only seen 'sometimes.'

"2. That when seen they are due to insufficient optical power or bad definition."

In writing that review, I tried to avoid the assertion of any proposition I could not fully sustain, and therefore very willingly give the evidence on which these propositions rest. At the outset, however, I beg leave to call especial attention to the fact that I did not assert the second in an absolute manner, but only said that it was "indicated" by observations and experiments.

The first proposition is sustained by the fact that at the last transit of Mercury, the majority of those observers who have described the phenomena saw neither ligament nor distortion, but only the geometrical phenomena of contact, the planet preserving its rotundity to the last.

The following is a statistical summary of the evidence on both sides:—Among the numerous English observations published in the monthly notices, fourteen describe the phenomena. Of these three saw the phenomena go on regularly, while eleven saw ligament, black drop, or distortion either before or after the contact. Among these eleven there is little agreement as to the exact nature of the distortion. Owing to the low altitude of the sun in England, I take it that the atmosphere was much less favourable than on the Continent.

At Marseilles Le Verrier saw the black drop. He used a seven-inch glass, of which both the centre and circumference were covered by a screen, which is sufficient to account for the phenomenon by the diffraction thus produced. Mr. Stephen, who observed at the same place with a very large reflector, "déclare n'avoir rien vu de pareil." *

Of the five observers at the Paris Observatory, Le Verrier says† :—"Les observateurs ont remarqué qu'il ne s'est rien présenté de particulier, ni au moment du contact intérieur, ni après ce contact. Mercure a touché le bord du Soleil en amincessant progressivement le filet de lumière, mais sans produire le phénomène de la goutte." Le Verrier was, therefore, so far as we know, the only observer in France who saw the black drop.

At Madrid Ventosa may have seen several black drops "tout-à-coup." His description, however, is rather obscure.‡

At Lund the egress was observed by Duner under very favourable circumstances with a nine-inch glass. He says§ :—"Die Bilder waren sehr ruhig, und die innere Berührung geschah in der Weise, dass der Lichtfaden Zwischen den Rändern des Mercurs und der Sonne erst dann brach als seine Breite verschwindend klein geworden war. Es zeigte keine Spur einer Verdrehung der Bilder oder des von anderen Beobachtern erwähnten schwarzen Tropfens."

At Pulkowa fourteen observers observed the egress. I learn that not one saw anything but the geometrical phenomena of contact.

To avoid a tedious collation of accounts which nearly all say the same thing, I remark that only two observers on the Continent saw any abnormal phenomena, namely, Kaiser at Leiden, and Oppolzer at Vienna. The first saw an elongation of the planet, which he thought might be due to maladjustment of his instrument.‖ The second saw the sun's limb pushed out by that of Mercury, so that apparent contact took place before the breaking of the thread of light.**

Summing up all the accounts, I find the result to be:—

Total number of observers who describe phenomena .	39
Number who saw the planet remain perfectly round, and the phenomena of contact occur with entire regularity, and without distortion, ligament, or drop	24
Number who saw ligament, distortion, one or more drops, or other abnormal phenomena	15

The twenty or thirty observers who do not describe the phenomena probably saw nothing abnormal, but they are not counted in the above list.

The first proposition is, I conceive, fully established by the statistical facts cited.

Passing now to the second, it may be remarked that when different observers give different descriptions of the same

* Comptes Rendus, 1868, ii., p. 921-924.
† Ibid. p. 948.
‡ Astronomische Nachrichten, vol. lxxii., p. 356.
§ Ibid, p. 378.
‖ Ibid, vol. lxxiii., p. 214.
** Ibid, vol. lxxii., p. 347.

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phenomena, there must be some corresponding difference in the circumstances of observation, and that when among five observers three see the phenomena exactly as we know they are, while two see them as we know they are not, and even then do not agree between themselves, there is a strong presumption that the latter do not see them rightly. I am aware of but a single attempt to determine experimentally the causes why one-third of the observers of the late transit, and many observers of former transits, saw the planet distorted, namely, that of Wolf and André, to which Mr. Stone alludes. They found, in observations of artificial transits under various circumstances, that when they used a telescope of at least twenty centimetres aperture, with a good object glass, well adjusted to focus, they saw only the geometrical phenomena of contact, while, if the object glass was small, or not well corrected for aberration, or not well adjusted to focus, they saw the phenomena of distortion.*

In the absence of farther investigation, which is much to be desired, these results seem to me, at least, to "indicate" that the phenomena in question are due to insufficient optical power, or bad definition either in the object glass or the atmosphere. At the same time I by no means insist on this proposition as established, and it is a great defect in the experiments in question that they do not extend to the effects of using shades of different degrees of darkness in observing the sun; but of this anon.

In his letter Mr. Stone quotes the observations of Chappe, Wales, Dymond, &c., in 1769; but I cannot admit that they bear strongly on either of the points in question, till we have some better evidence than now exists that their object glasses were such as Clarke or Foucault would call good.

Again, the argument from irradiation, if it proves anything, proves too much. I do not see why, upon the theory of Mr. Stone, the distortion should not always be seen. To be satisfactory, any theory of the matter must explain why it is that A, B, C, &c., see the phenomena, while X, Y, Z, &c., do not, and that of Wolf and André is the only one which does this.

Mr. Stone objects to the experiments of the Paris astronomers, that their disc was not sufficiently illuminated to exhibit any optical enlargement. I do not know his authority for this assumption, but, whether well founded or not, it seems to me that if the sun were viewed through a dark glass, it would present the same optical phenomena of irradiation with a disc so illuminated as to appear of the same brilliancy with the darkened sun. Thus, in the absence of evidence to the contrary, the Paris experiments may be taken as showing how the phenomena would present themselves in the case of the sun viewed through a shade of a certain (unknown) degree of darkness.

Before we can make any application of the theory of irradiation to the phenomena of contact, we require to know whether the irradiation of an extremely minute thread of light, darkened so as to be barely visible, is the same with that of a large disc. I am decidedly of opinion that it is not, and, if not, the fact that the sun's disc is optically enlarged by the telescope or the eye of the observer, cannot be directly applied to the phenomena of a transit.

To sum up my views;—neither Mr. Stowe nor any one else will claim that the ligament he saw before the time of internal contact was a celestial reality—he considers it a result of irradiation, but whether of telescopic or purely ocular irradiation I do not understand. If the former, this is simply a species of bad definition, and there is little difference between us. I also admit, on my part, that if the telescope and the eye are such that from any cause whatever an exceedingly thin thread of light presents itself to the sense as a band several times thicker than it really is, then, as the real thread becomes invisible, the seeming band will appear to be broken through by what some may consider a ligament and others a black drop, and the really sharp cusps will seem to be rounded off at their points. If I rightly understand Mr. Stone, he holds that these results of the thickening of the thread of light by what he considers irradiation are unavoidable. But this view is conclusively negatived by the fact that they actually were avoided by a large majority of the observers of the late transit. Admitting, then, that these spurious phenomena are not unavoidable, it matters little whether we call their cause irradiation or bad definition, though it is important that we should know its exact nature. The only attempt I know of to determine this is that of Wolf and André, and their results seem to me so nearly in accordance with what we should expect, as to be quite worthy of acceptance, at least in the total absence of rebutting evidence.

SIMON NEWCOMB

* Comptes Rendus, 1868, i. p. 921.

Solar Parallax

I HAVE waited somewhat anxiously for Prof. Newcomb's statement of the errors in a chapter on the Sun's distance ("The Sun," Chapter I.). His review was certainly so worded as to imply very gross inaccuracy, and his explanatory letter, in which he remarked that more than a column of NATURE would be needed for the mere record of my errors, did not improve matters. This morning I have received his notes. The errors enumerated amount but to seven in all; I will leave your readers to judge of their importance.

1. At p. 50, I assign to Hansen's letter of 1854 the announcement of the value 8″·9159 for the solar parallax; whereas this value was not announced by Hansen until 1863. *Tanquam referat.* Hansen's priority remains unaffected by the change.

2. At p. 53, I mention that Prof. Newcomb deduced a value of 8″·84 (probably a misprint for 8″·81) for the solar parallax by a certain method. His real result was 8″·809. Again my comment is *tanquam referat.*

3. At p. 53, Foucault's "parallax is given as 8″·942, whereas the result actually deduced was 8″·86." The matter again is utterly insignificant; but it chances that I have not given Foucault's estimate of the parallax as 8″·942. I remark only that if Foucault's estimate of the velocity of light is correct, the parallax would be 8″·942. I deduced this result by a calculation made on my thumb-nail as I wrote. It is correct, however, and Foucault's was not.

4. At p. 59, I say that Mr. Stone deduced the solar parallax from observations of Mars made at Greenwich alone, and then by combining these observations with others deduced the solar parallax at 8″·943. Now, Prof. Newcomb says that he "finds no discussion of the observations at Greenwich alone, *in the paper here referred to.*" But I refer to no paper whatever. A rough calculation of the parallax was certainly made from the Greenwich observations alone, though, as Mr. Dunkin remarks at p. 507 of his edition of "Lardner's Astronomy," "the observations by this method (single-station observations) were comparatively unsuccessful," "owing to unfavourable weather at Greenwich." Apart from the facts, which fully justify my statement—what could the correction be worth in any case? Only the final result was insisted upon.

In a note on this matter, Prof. Newcomb makes "in passing" the really important observation that the method of determining the sun's distance by observations on Mars from a single station was applied by the Bonds as far back as 1849. Mr. Carrington had already told me that he believed the Bonds had anticipated the Astronomer Royal. I wrote to Prof. Young asking for further information, and was waiting for his reply, I am obliged to Prof. Newcomb for aiding me in this matter. The priority of the Bonds in this matter should certainly be more widely known than it is.

5. At p. 61. This is a very curious correction. I speak of Prof. Newcomb as having successfully treated the problem which was *afterwards* discussed by Mr. Stone; and he remarks that he knows nothing of the matter, and has read my statement with great bewilderment. I am not responsible for it. There is a letter in the *Astronomical Register* for December 1868, signed only "P. S.," but with unmistakable internal evidence of coming from the Astronomer Royal for Scotland, in which the following passage occurs:—"I must not say a word about the pyramid sun-distance here, or my letter will never be allowed to see the light; something, however, on the score of modern justice to our contemporaries I must beg leave to put in. Admirable is the praise given to Mr. Stone, and worthy, in so far, the credit abundantly bestowed on him at every step of the undertaking; but why is there not one word about Prof. Simon Newcomb, of America, having already gone over that same problem similarly, and published the results a year sooner?" Of course, as Prof. Newcomb now writes that "he has no recollection of ever having made any independent investigation of the observations of the transit of Venus," Prof. Piazzi Smyth was mistaken, and "the abundant discussions of Prof. Newcomb's paper in various northern scientific societies last winter" (so speaks Smyth) were founded on some misconception of its purport. But Prof. Smyth's statements were permitted to remain uncorrected;—*hinc illæ lacrymæ.*

6. At pp. 61, 62. "The distortion of Venus at the time of internal contact is described as an ever-present phenomenon, and the apparent formation of the ligament as contemporaneous with true internal contact." If what I say in pp. 61, 62 admits of being so misinterpreted (which I question), the same cannot be said of

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